educational materials. Furthermore, across all sites, the readability of approximately 50% of the materials exceeded the average reading level, indicating that many of the materials are still inaccessible to the majority of the target population.

In summary, although considerable progress is evident in making HIV/AIDS educational materials more accessible, more work needs to be done to further realize this goal consistently. It must be noted that the four sites selected for this study were aware of the readability issue when selecting their educational materials. Because it is likely that greater discrepancies exist at sites that are not as cognizant of the need for reading level matching, it appears important to replicate this study at other sites. □

Mark E. Johnson, PhD
Stephen L. Mailloux, MS
Dennis G. Fisher, PhD

The authors are with the Department of Psychology, University of Alaska, Anchorage.

Requests for reprints should be sent to Mark E. Johnson, PhD, University of Alaska, Department of Psychology, 3211 Providence Dr, Anchorage, AK 99508.

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Can Herd Immunity Influence the Assessment of Vaccine Efficacy in Nonrandomized Studies?

Early in this century, F. S. Lister[1] made the following statement in his article on large pneumococcal vaccination trials in South African gold miners:

> In the system hitherto employed on the Rand for assaying the results of prophylactic inoculation against Pneumococcus a certain advantage conferred upon the uninoculated has, I think, been overlooked. If Pneumonia is spread, as I believe it to be, either directly from case to case or through the agency of carriers it follows that inoculation of half the inhabitants of a Native compound may interrupt the chain, not only of actual pneumonia patients, but also of carriers; if the inoculation achieves this it is obvious that the uninoculated half of the population will reap an advantage which is not allowed for in the calculations.

Lister[1] was describing the phenomenon of herd immunity, defined as the indirect protection of susceptible individuals by immunes.[2,3] He suggested that this indirect protection should be included when evaluating the total public health advantage conferred by a vaccine. However, herd immunity may also influence the assessment of vaccine efficacy for an individual. Let Iu and Iv be the disease incidence in the nonvaccinated and vaccinated. In follow-up studies, vaccine efficacy is calculated as $[(Iu - Iv)/Iu] \times 100\%$. In case–control studies, it can be estimated as $[(RR - 1)/RR] \times 100\%$, where RR is the estimated risk of disease in the unvaccinated.

How can herd immunity influence the measurement of vaccine efficacy? Herd immunity not only decreases the disease frequency in the nonimmunized but also affects the vaccinated (if the vaccine is not 100% effective). If disease frequency decreases in the vaccinated and unvaccinated to the same relative amount (e.g., by 0.25), no bias would occur:

$$\text{Vaccine Efficacy} = \frac{0.25\,lu - 0.25\,lv}{0.25\,lv}$$

This can be expected if a vaccine is distributed randomly, because the probability of encountering the infectious agent is then the same for the vaccinated and nonvaccinated. However, in the absence of randomization, clustering of vaccinated persons may occur.[2] To the extent that the vaccine reduces an individual's ability to transmit the infection or increases the intensity of exposure required to produce disease, the incidence in vaccinated persons will be lower than had vaccination been administered randomly. Correspondingly, clustering of nonvaccinated persons will prevent each of them from obtaining the "benefit" of exposure to persons who have been vaccinated. As a result, the incidence in nonvaccinated persons would be higher than had there been randomization. Therefore, it is likely that if vaccine efficacy is estimated at the level of a population[4] containing exposed and nonexposed individuals, it will truly differ depending on whether or not the vaccine is administered at random. However, if vaccine efficacy is viewed instead as the degree of protection afforded to an individual who has been exposed to the infectious agent, clustering of vaccine status in the study population has the potential to produce estimates of vaccine efficacy that are falsely high.

Clustering of individuals who receive a given preventive intervention other than a vaccine has a similar potential to alter the efficacy of that intervention. For example, a cigarette smoking cessation program could well be more effective when administered to people who live or work together than when administered to people at random if one factor that interferes with a person's cessation is seeing his or her relatives and coworkers smoking cigarettes. □

Kathrin Mühlemann, MD, PhD
Noel S. Weiss, MD, DrPH

The authors are with the Department of Epidemiology, School of Public Health, University of Washington, Seattle. Kathrin Mühlemann is also with the Department of Medical Microbiology, University of Berne, Berne, Switzerland.

Requests for reprints should be sent to Kathrin Mühlemann, Department of Medical Microbiology, Division Epidemiology of Infectious Diseases, University of Berne, Friedbühlstrasse 51, CH-3010 Berne, Switzerland.

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The Validity of Recalled Birthweight in Developing Countries

Birthweight is an important predictor or confounding variable in many areas of research such as child growth and development,[1] and it may also be important in studies of adult disease.[2] However, studies often begin in childhood or later, so data on birthweight must be obtained retrospectively. Hospital records are the best source for birthweight data. However, particularly in developing countries, some records may be missing, or records may not exist. In these cases, recalled data have to be substituted, but the validity of such data has not been adequately described.

As part of a longitudinal study of child growth and development among low-income families in Kingston, Jamaica, we collected birthweight data by mother's recall when the children were 3 to 4 years of age. Another group of children were enrolled in the study at 7 to 8 years of age, and recalled birthweights were obtained at this point. In order to determine the validity of the recalled birthweights, we traced the children's hospital records. Records were available for 111 of 154 (72%) children for whom recalled birthweight data had been obtained at 3 to 4 years of age and for 132 of 182 (73%) children for whom data were first obtained at 7 to 8 years of age. Birthweights were recorded and recalled in pounds and ounces. Weights ending in 0, 4, 8, or 12 oz occurred somewhat more frequently than would be expected in both the hospital records and mothers' reports, although there was no evidence of rounding up by the mothers. Weights were converted to kilograms for analysis.

TABLE 1—Mean Birthweight of Children in Kingston, Jamaica, from Hospital Records and Mothers' Recall

	Child's Age at Recall, y			
	3–4 (n = 111)		7–8 (n = 132)	
	Mean	SD	Mean	SD
Recorded birthweight, kg	2.98	0.54	3.28	0.47
Recalled birthweight, kg	3.03	0.59	3.35	0.52[a]
Mean difference,[b] kg	0.05	0.31	0.07	0.34

[a]Recalled significantly greater than recorded ($P < .05$).
[b]Recalled minus recorded.

The Pearson product–moment correlations between hospital records and birthweights recalled at ages 3 to 4 years and 7 to 8 years were .85 and .77, respectively. Mean recalled and recorded birthweights and the mean difference between the hospital records and recalled data are shown in Table 1. In both cases, mothers' recall of their children's birthweight was slightly higher than the recorded values.

We collected information on education and verbal IQ, using the Peabody Picture Vocabulary Test, for the mothers who reported on birthweight at age 3 to 4 years. The majority of the women (81%) had a primary-level education but had not completed secondary-level education. The difference between recalled and recorded birthweights was not greater (1) in mothers with less education or (2) in the 50 mothers with Peabody Picture Vocabulary Test scores of less than 80 relative to the 61 mothers with scores of 80 or higher.

Our results show that mothers in Jamaica (and, perhaps, in other developing countries) whose babies were born in hospitals can recall their children's birthweight but that the accuracy of recall declines with time since birth. Recalled birthweights can therefore be used in instances in which records are not available but should be obtained at as early an age as possible. □

Pamela Gaskin
Susan P. Walker, PhD
Terrence E. Forrester, MBBS, PhD
Sally M. Grantham-McGregor, MD

The authors are with the Tropical Metabolism Research Unit, University of the West Indies, Kingston, Jamaica.

Requests for reprints should be sent to Susan Walker, PhD, Tropical Metabolism Research Unit, University of the West Indies, Mona, Kingston 7, Jamaica, West Indies.

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